SOME ASPECTS OF MODERN TAXONOMY*

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It should not be necessary to define for the readers of this journal the word taxonomy. Perhaps we have not each taken the trouble to think out what it means to us, but we nevertheless understand it well enough. I was therefore somewhat surprised recently to find out that I was attaching to the word in my own thoughts certain meanings which it did not hold for some other taxonomists to whom I talked. So I desire to make it clear that I am now using the terms taxonomy, classification, and systematics as absolutely synonymous, since they all inherently refer to the science of arrangement or classification.

The study of taxonomy in its broadest sense is probably the oldest branch of biology or natural history as well as the basis for all the other branches, since the first step in obtaining any knowledge of things about us is to discriminate between them and to learn to recognize them. It is therefore natural that for the first one hundred years or more of the existence of this branch as a science, it was concerned primarily with the segregation and recognition of species. The direct results of this trend are sometimes underestimated, for they include the following among others: First, taxonomists made known some idea at least of the tremendous number of organisms that exist or have existed upon the earth. This fact has had a very great influence on some of man's long-established and much-cherished conceptions. In particular, it gave a very great impetus to the warfare of science with theology. Second, the knowledge accumulated by the taxonomist was the principal basis for Darwin's "Theory of Evolution." It is not necessary for me to point out the tremendous affect on human thought which was produced by that series of studies. Third, taxonomists have built up an enormous mass of knowledge which is in some degree classified and available. The system is far from perfect, we must admit, and many of its parts are of very inferior quality, but, nevertheless, the general pattern has stood the test of time and has proved its usefulness. Fourth,

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taxonomy has furnished to other biologists many of the fundamental questions to which they are seeking answers. The geneticists and experimental biologists spend much of their time on problems which are fundamental to taxonomy today and yet were recognized because of the implications of studies on classification and evolution.

We might multiply these examples at some length, but surely this is sufficient to show that taxonomy during its early years was a worthy subject for research, and that it did actually contribute greatly to the advance of knowledge in many fields.

As I have said, there was a time when taxonomists made large contributions to scientific thought through the mere naming and cataloging of species. As the other branches of biology developed, the relative importance of this study of faunas and floras diminished, not because fewer people worked at it, not because there was less work to be done, not because there were fewer contributions to scientific thought to be made, but simply because the growth of these other branches was more rapid, and they surpassed it in popularity and possibly in the quality of the work. We must then consider whether taxonomy is no longer capable of contributing to biologic science, whether it has lost its position of fundamental importance among all the branches of biology.

In a practical way, then, let us outline the place of taxonomy at the present time. Consider the plight of experimental biologists, geneticists, students of geographical distribution, stratigraphers, and workers in the applied natural sciences if they had no means of recognizing and recording the various species with which they deal. It would be absolutely necessary for them to develop for themselves a system that would serve them and they would therein become systematists. How could the tremendous mass of experience be recorded and consulted without the taxonomists to name, identify, and classify the organisms with which all the others deal. A few examples should not be out of place here. Many problems of the geologist have been solved at least in part by the taxonomist. Properly used, the distribution of animals can aid in the problem of the distribution of ancient land masses and the seas of past geologic eras. Stratigraphy, or the correlation of rock formations, depends in large part on the recognition and classification of fossil organisms. The physical anthropologist, with his problems of human races and human origins, is merely a specialized taxonomist working in a very limited field. Many problems of the geneticist, such as the recognition and separation of mutants and the experimental unfolding of the processes of evolution, are really specialized problems in taxonomy. I have not mentioned the sciences of comparative anatomy, embryology, and evolution which are so closely interwoven into taxonomy that frequently they must be considered to be part of it.

There remains a branch of biology which is of unusual interest to many entomologists. This is applied biology. Just as the discrimination of species is the basis of systematic work, it is also the starting point of many of the problems of the economic entomologist. If an insect pest be discovered, taxonomy tells us whether it is native or introduced, what its natural enemies are, where they will be found, what its normal distribution is, and many other things. Without taxonomy how could we have biological control, which depends upon identification of both the pest and the parasite or predator.

We have then several aims in systematic biology which are also our basis for a claim to recognition as one of the important sciences. It is our aim first to name and describe species so that they can be recognized and referred to, so that we may study them and catalog them, and assemble data about them. It is our second aim to discover through any means at our disposal the facts of descent and blood-relationship between species. And our third aim is to arrange these facts and these species into a classification or scheme of arrangement which will express as nearly as can be the relationships and the lines of descent of these species.

It is obvious that if we merely asemble a multitude of data concerning a multitude of species, we ourselves, not to mention scientists in other fields, will be unable to comprehend the larger implications of our discoveries, the interrelation of them, and their vast significance in related studies unless we arrange them in some sort of order, one based on some fundamental concept to which all are related. The concept which we have used for this is the theory of evolution, the assumption of the community of descent of organisms. We will return to this subject of classifications after considering the means to be employed in fulfilling the aims we have outlined.

In the study of the relationships of insects, which we call taxonomy, data from various sources have been used. Among these we find characters of morphology, of geographical distribution, of geological distribution, of genetics, of ontogeny or development, of ecology, of physiology (which includes chemical and serological studies), of host-parasite relationships, of teratology (or deformities), and of experiment. All of these have been used in defining species or in building classifications, but by far the greater number of species and classifications are based entirely upon the first one—morphology. This is at it should be, since it has been found that none of the others yields as readily as complete a picture of relationships as does structure. The other fields are employed in special cases in which morphological data are not sufficient. There is a real danger, however, that taxonomists will forget that morphological data do not invariably furnish us with the complete picture. We should remember that situations can easily arise in which data from the other fields can be properly and usefully utilized to supplement the characters of morphology.

Still more important, however, is an understanding of the inescapable fact that the taxonomist is absolutely bound to consider all the data that may be of value in whatever problem confronts him. If we had, for example, spread out before us, all the species that exist on the earth of a certain group of animals, it would suffice for some purposes to select one or two characters that distinguish each from its fellows, and consider that these were enough to make the species known. In actual practice, however, we can never say with certainty that we have all the species that exist before us and the best of us will often not be able to anticipate what the missing ones will be like. It therefore becomes necessary for us to record all the characters of each species that can be of use in separating it from the others (whether known or not). In most cases and particularly in the case of categories higher than species, a careful study of comparative morphology of the group will show definitely which characters are of sufficient constancy to be used for separating the known groups, and these are the ones which we must record.

The ideal, then, is to record in each instance all the characters on the insect which the study of comparative morphology shows to be significant for the systematic work at hand. It should not be necessary for me to point out how far from this goal nearly all of our taxonomic work today really is. But inasmuch as the commonly accepted standards do not come even close to fulfilling the above requirement, let us pry a little into the reason for this failure.

During the early years of the study of taxonomy the investigation of insects was strictly limited by the equipment available. Microscopes such as we use today were unknown, and technique of various sorts had not been discovered. Many of the early taxonomists strived toward the goal of recording all the characters that they could see, and, because of the limiting factors mentioned above, a certain standard of description was gradually set up. Taxonomists became used to these standards and when better apparatus and techniques were devised they were not commonly utilized. The result is that the large majority of our systematic work today is at exactly the level of much of the better work of 100 years ago. For example, it is difficult to find among modern studies on Coleoptera one which can stand comparison with that of Erichson in 1840. This is not an exceptional case, for the majority of large groups of insects are being studied today in exactly the same way and in only slightly greater detail than they were 50 or 100 years ago.

Two things seem to be responsible for this situation. The first is our reticence to change our methods of study. It is not possible to examine the intersegmental membranes of the abdomen of a beetle, or the waxy capsule of a scale-insect, or the musculature of any insect, or the minute dermal organs which are so commonly present but so seldom seen, or any of hundreds of other structures, without subjecting the insect to some preparation or technique which is not commonly employed. Yet these structures are of great significance in nearly every group in which they have been studied and often are clearly worthy of consideration in our scheme of classification. We have simply been content with the established way of doing things and have forgotten our ideal of constructing a classification that will reflect relationships and which, therefore, must utilize all possible characters.

The second factor which has held our descriptions down to the old standards is the lack of knowledge of comparative morphology. Is there a single large order of insects in which the fundamental nature of all the parts has been worked out and in which the variation of each character throughout the group has been determined? It is certainly not so in the beetles, one of our best known orders in some respects. If it is true of any order the fact has escaped me. Several smaller groups, such as the Coccoidea, do come near to this ideal.

Ten years ago it was not known that the order Coleoptera contains two radically different types of thoracic structure. Even at the present time there are numerous questions of homology that have not been settled in this order or any of the others. If we should look at a complete series of drawings of the morphological details of a beetle, I think you would be surprised at the number of structures which have not been used in classification, and yet this condition is typical of nearly all the orders.

Let me recall to you the ideal which I mentioned before for finding the characters we are to use in reconstructing the relationships of species. We must use all the characters which the study of comparative morphology shows to be significant. We are exceedingly far from that ideal.

In addition to the segregation and description of species the taxonomist should be engaged in something much more far-reaching in science, something which will be more universally accepted as a truly scientific endeavor. This is classification. It is the arrangement of species into groups to show their relationships. The groups are then brought together into larger groups and so on, until the degree of relationship is expressed by the category in which union takes place. In spite of the fact that the organizing of our data into systems or classifications, into a form which will make it useful in other branches of science, is much the most important part of taxonomy from the point of view of the advance of knowledge, entomologists have been very slow to make contributions of this sort. If a certain group of animals, for example an order, has been so thoroughly studied that a complete classification is available, with definitely known and described categories throughout, all we would need to do upon the discovery of a new species would be to describe it and place it in its proper genus. This would automatically place it in the rest of the system. In reality this is the way in which many taxonomists appear to work. The assignment of a new species to a genus is taken to be sufficient indication of its relationships. I venture to say that there is not a single large order of insects in which there is more than the flimsiest classification in the sense that I have tried to give to that word.

Let me illustrate with an example from my own field, the Staphylinidæ. In the very large subfamily Aleocharinæ there has been a large amount of work done by several profilic taxonomists. They have established more than 1000 genera, which contain over 7000 species. New species are being added continually and each is being placed by its describer in what he deems to be its proper genus. There is nothing unusual in this; it is being duplicated, perhaps on a smaller scale, in many other groups of insects. However, if we take the trouble to probe more deeply, we may be surprised to find that not a single one of these 1000 genera has ever been *adequately* described, and many of them not described at all, being based merely upon known species. And further, when we examine other groups we find that this is not an exception drawn from a badly neglected group but is in fact the ''normal'' condition, or at least the ''usual'' one. In a family which is as well known as the Coccidæ, the scale-insects, a study of the genera is even now being published that will for the first time enable us to make generic assignments with certainty. The large amount of taxonomic work which has been done in this group and its great economic importance would have led us to imagine that its classification must by now be on a firm basis, yet a recent article on the subject states: ''. . . the student of the Coccoidea . . . is forced to wander in a maze of generic names the application of most of which can not be determined from the existing literature.'' If this is true in the Coccidæ, where can we find a group which can claim to be better known in these respects. Certainly not anywhere in the Coleoptera.

How can we have any confidence in the validity of the horde of new species that are described each year if we know in advance that their generic assignments are based entirely upon the author's conception of the genus to which he assigns it. How often is the same species described in different genera by different workers, merely because the genera have never been firmly established? As one would expect, it happens with great frequency in groups of wide distribution and accounts for a large part of our overburdened synonymy.

You may think that I have painted a very dark picture. But it is a picture of a condition which exists and which will continue to exist until taxonomists take the necessary steps to correct it. I am glad to be able to say that there is evidence of increasing realization of the seriousness of this condition, and there are an increasing number of attempts to help correct it. I have mentioned the study of the genera of the Coccidæ, and we may note also an increasing number of studies of genotypes and of groups of genera. There have even been a few studies on the principles and bases of higher classification. This is the track that we must follow if taxonomy is not to continue to merit the bad reputation it has acquired among biologists. We must be more than mere describers of new species and lawyers on arbitrary points of procedure.

There is one more point that should be mentioned in this regard. A division of labor is not the solution of this problem. It will not suffice for us to describe new species *ad infinitum* and leave the classification to someone else. We cannot possibly claim to know that a species is new unless we know definitely where it belongs in our classification. And we will have a hard time to justify our labors to science in general if we do not complete our work so that the results are available for others to use.

If I have succeeded in convincing you that our taxonomy has fallen far short of its ideal and that we come close to deserving the scorn of our fellow biologists, if I have established in your minds the idea that taxonomy must be more than the mere description of new species, then you will ask what is to be done and how can we do it. My answer is, of course, that we must make classification a major part of our work; we must arrange our knowledge, as our species, in a system which will express what we have been able to discover, by all means in our power, of their interrelationships, of their origins and potentialities, and consequently of their evolution. This is the logical goal of systematic work and one which is in all respects fully worthy of our endeavors. If we can attain this goal we will find that most of our other problems in taxonomy, such as the identification of species, will be solved as by-products of the major problems. Let us see, then, what a classification is and how it is made.

A classification is a grouping or arrangement of things with regard to some group of attributes. We can classify insects according to their food, their place or means of living, their distribution in space or in time; or by their structure. In taxonomy we are interested primarily in a classification based upon amount of similarity in structure because we believe that this will give us the truest and most complete picture of lines of descent and degree of relationship.

There are two principal methods of recording a scheme of classification. They have different uses and different advantages and disadvantages. The first is a purely linear arrangement. We place the most primitive at the first of our list, next to these the ones which resemble them most, then the next, and so on to the most highly specialized. Our arrangement is rather arbitrary because one group must follow after another. Relationship can be shown with only two other groups, the one preceding and the one following, and it is not possible to give any indication of the degree of relationship, the amount of similarity. This system is most commonly employed because it is readily adapted to printing. Examples are to be found in all our catalogs and check-lists and all our textbooks of taxonomy.

The other system of recording a classification is by means of a branching arrangement, usually called a "tree." There are two kinds of trees in use. The most familiar is that used in paleontology to indicate the relationships of animals in time. As one passes down the time-scale the various groups merge to form a tree which indicates in some measure the degree of relationship, the time at which the separation of the two groups occurred, the number and proximity of related groups, the lines of descent, and perhaps even the ancestors of each group. This tree is generally based upon very meager information, but is useful and illuminating in proportion to its accuracy.

The type of tree which can help us most is one which we see very seldom in entomological literature. It is not of exceedingly great importance in itself but serves several very useful purposes—or might serve them. Its preparation would crystallize and demonstrate many of the broader aspects of classification in the mind of the creator of the tree. It would enable other entomologists as well as other scientists in general to see the results of the detailed work of taxonomy. This type of tree is in a strict sense "a classification." I do not want to start a discussion of what a species is, so let us take the word species as each of you would define it for yourself. Among these species those which have certain characters in common we group together into a genus; genera which are more like each other than they are like still others we group together into a family; and so on through orders, classes, phyla, and kingdoms. This is a classification. If we examine simply the species, they appear in a linear arrangement but the other categories can show us the degree of interrelationship and can bring this meaningless series of species into a system in which each is related by one degree or another with every other.

Such classification as this gives us a broad picture which includes not only our own species but all the others as well, relating them to each other and to the entire scheme of life and of evolution. Such a classification can help to demonstrate to us inconsistencies in our use of such categories as genus and species, it can demonstrate the need for a real understanding of the higher categories and the limits of each.

It is obviously not practicable to draw diagrams of this sort for all the groups of insects. But it is not the diagram that is of value so much as it is the idea of relating our groups to each other by means of successively higher categories. For example: In one subfamily of the Staphylinidæ we have four readily recognized groups which have all been named as tribes, thus: Xantholinini, Staphylinini, Xanthopygini, and Quediini. There is no question that these groups exist and that they are more like each other than any of them is like any of the other subfamilies of the Staphylinidæ. At first glance then it would seem to be adequate to rank them all as tribes as has been done heretofore.

However, careful examination of many characters shows that the Xantholinini differ more from any of the others than they do from themselves. Some writers, recognizing this, have made it a separate subfamily, but I have already pointed out that it is more like the rest of the subfamily Staphylininæ. If we separate it, we obscure the fact of its similarity in subfamily characters, and if we make it a tribe we obscure the fact of its greater divergence.

What is really needed in this case is another category between subfamily and tribe—we may use supertribe. This is what we get then: The subfamily Staphylininæ composed of two supertribes, the Xantholinina and the Staphylinina. The Xantholinina contains only a single tribe, the Xantholinini, but the Staphylinina contains three tribes, the Staphylinini, the Xanthopygini, and the Quediini. Now we are able to see how much each of these groups resembles the other and how great the degree of difference is in each case.

The point I wish to show here is that there is much more to taxonomy than the segregation and naming of species. These should be used merely as tools to enable us to handle the groups while we combine and arrange them into a scheme which will show their interrelationships. How are we to know whether a particular group of specimens is a species unless we know in just what degree they differ from or resemble the numerous other groups we call species? We must know more than just the tail-end of the scheme of evolution if we are to be able to say that a group of individuals is a species, a subspecies, a genus, or some other category.

The study of classifications will not, of course, be a cure-all for our problems of speciation, of descent, or of relationship, but it will go a long way toward giving us an understanding of how groups relate to each other, of what rank should be assigned to each group, and of what the course of evolution of these groups has probably been.

It is more the attitude of mind which is important, and when new facts are brought to light which affect our established system, we must not only be prepared to accept them for what they are worth, but we must have a background of thought which will enable us to see how they affect our ideas. To many a taxonomist the idea that he has consistently misused a category such as a genus or a species, giving it a higher or a lower rank than is consistent with the facts, is something that he is utterly unable to understand. The standard that he uses is so firmly fixed in his mind as the true standard of that category (let us say of a species) that he cannot believe that any facts could upset the standard.

On the other hand we have a taxonomist who is used to thinking in terms of a more or less complete classification, where the categories indicate the degree of known relationship. If evidence is discovered that shows that his species are in reality only subspecies (or races), he is not so likely to object because the groups are still intact and usable but are simply changed slightly in the system and a further opportunity is opened for showing the development of the system.

This subject of classification is a very broad one. It has ramifications in many directions. I have touched on certain phases only, without intending to give these an exclusive claim to importance. But let me summarize some of the principles that should be basic in the study of taxonomy. First of all, we *must* consider *all* the available data. This means that we must use characters from whatever field of science we can; in the case of morphology, which is most important, we must aim to use all the structures on the insect, whether they are obscure or hard to examine or little known, and our only guide here shall be that comparative studies must show them to be of value for what we are trying to do. This principle, then, demands a complete knowledge of morphology and homology as basic to any study of systematics.

The second principle is this. Regardless of other considerations, we must use methods of study or procedures in working that will give the most complete and accurate results. We must not let our methods depend on the habits of the past. The important thing is not to follow any set procedure, but to treat every case on its merits and requirements and to employ every means possible to arrive at the complete truth. For example, if we find an important character on an insect that requires dissection of our specimens, we cannot neglect to dissect them if we are to be worthy of the name of scientist. Or if we should find that facts from genetics, ecology, development, or paleontology are significant, we must not fail to consider them adequately.

Our third basic step is to carry our work on to its logical conclusion, at least as far as taxonomy is concerned. If we merely describe species and genera and are not able to show exactly how they relate to other known species and genera, where they fit into the whole structure of evolution, how they add to our knowledge of the whole scheme of life, we will have missed our principal opportunity to make real contributions to science. We speak of the species that are "known to science," yet how many of these are really known in any sense beyond the publication of a name and description? Many are, of course, but not a very large percentage of the enormous numbers named. The placing of a species in a genus, and the genus in a family, without very careful consideration of the foundations of these groups, adds little to the general picture of relationships which we are attempting to set up.

This brings us to the fourth, and in some ways the most important, principle. We must make our results available for other scientists to see and understand. Endless series of new species and new genera not only do not aid the workers in other branches of science but actually serve to make taxonomy appear to them as meaningless and purposeless. I'm sure you will agree that this is the condition in which we find ourselves today, for many other biologists have no conception of the part which taxonomy can play in science and are inclined to look upon taxonomists merely as egotists trying to attach their own names as authors of new species. Taxonomy was once much more than that and can be again, but we will have to enlarge our horizons, raise many of our standards of thinking, and make our results available and useful to the rest of science.